

molecule is nowhere defined (in a note on p. 57 a casual statement is made as to the meaning of the term); "Avogadro's law," which lies at the basis of the whole modern edifice of chemistry, is conspicuous by its absence; certain statements as to gaseous combination and to "volume weights" are made, it is true (p. 35), but these are incomplete and misleading.

When a theory of valency is raised on so slender and shifting a molecular foundation as is here laid, no wonder that the edifice should be a strange one; the definition of "atomicity" on pp. 54-55 is incomplete, and cannot be upheld by facts; the statement on p. 58, "it is then a law to which there are no real exceptions, that though the equivalence of an element may vary, it does so always by the addition or subtraction of an even number," is simply untrue. As an "important conclusion" from certain "facts" (? fancies) "on equivalence," it is stated that (p. 59) "a formula which possesses an uneven number of bonds or units of chemical affinity cannot possibly represent a molecule"; without minutely criticising the expression "bond or unit of chemical affinity," suffice it to say that such a formula as, according to Dr. Kemshead, cannot possibly represent a molecule, unfortunately does represent a molecule. The existence of the molecule NO is a case in point: *à propos* of this compound, there is a charming example of the author's method of treating chemical science as a collection of opinions of various authorities to be found in a footnote on p. 169.

Notes on the Crania of New England Indians. By Lucien Carr. From the Anniversary Memoirs of the Boston Society of Natural History, 1880.

THIS is one of the numerous contributions now being made towards our knowledge of the fast-disappearing race of North American Indians. The author, Mr. Lucien Carr, holds the office of Assistant Curator to the valuable Museum of American Archaeology and Ethnology at Cambridge, Mass., an institution owing its foundation to the liberality of Mr. Peabody, so well known in England by his benefactions to the London poor, and its scientific excellence to the zeal and organising power of its first curator, the late Dr. Jeffries Wyman, and of his successors.

The object of the present memoir is to collect together such information as is still to be obtained regarding the cranial characters of the native Indians of the New England States, the celebrated "five nations" of the early historians of America, who in consequence of their geographical position were among the first of the race to succumb to the inroads of European immigration. Measurements are given of 67 crania, of which 38 are assigned to males and 29 to females. The averages of these measurements give the following results:—A medium cranial capacity, *i.e.* 1436 cubic centimetres for the males and 1319 for the females. A latitudinal index of '759, showing mesaticephalism verging upon dolichocephalism. The altitudinal index exactly the same. The principal facial indices show orthognathism, with a strong tendency to mesognathism, a mesorhine nose (index 50), and slightly megaseme orbits (index 88 in the males, and 91 in the females). Although these are the average characters of the whole collection, very few, if any, of the individual crania are to be found presenting them. There is indeed no such uniformity among these skulls as may be seen in certain races, such as Eskimos, Bushmen, Fijians, Andamanese, or even Australians. Perhaps it could scarcely be expected in inhabitants of a large continent, presenting great diversities of climatic and other conditions, and with no natural barriers to free migration and intercourse. The examination of these skulls therefore confirms what has been often remarked before, that although in a broad sense the American Indians present a certain community of type, there is

great diversity in detail among them, the result probably of a long series of repetitions of the process of breaking up into distinct groups or tribes and reuniting in various combinations.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Conservation of Electricity

IN a recent communication to NATURE (vol. xxiv. p. 78) Prof. Silvanus P. Thompson very kindly mentions my latest memoir on "The Conservation of Electricity," and, as I am glad to find, confirms my views on this subject by stating that he has independently arrived at the same conclusions with myself.

As regards however the question of priority moved by Prof. Thompson, I think I ought to add that an earlier paper of mine, published five years ago, must have escaped Prof. Thompson's attention. This was printed as an abstract in the *Comptes rendus* of the Paris Academy of Sciences for June 19, 1876, under the title, "Extension du principe de Carnot à la théorie des phénomènes électriques. Équations différentielles générales de l'équilibre et du mouvement d'un système électrique réversible quelconque." I there enunciated the law of the Conservation of Electricity in the same terms as now, and also gave the same analytical method for applying it. I beg leave to quote as a proof the following explicit passage from this extract:—"L'équation $\int dm = 0$ a une autre signification plus simple; elle signifie

que de l'électricité peut se déplacer, mais ne peut jamais varier en quantité. Ce principe de la conservation de la quantité d'électricité a été admis par les physiciens dans tous les cas connus jusqu'ici, influence, frottement, etc. . . . Pour que $\int dm = 0$

pour tout cycle fermé, il faut que dm soit une différentielle parfaite." This method I had already applied in 1875 to the phenomena presented by mercury electrodes (*vide Annales de Chim. Phys.* 1875). In fact my latest memoir is merely a renewed attempt to draw, by means of new applications, the attention of physicists to a fact which I cannot help considering as important for the future, viz. that the principle of the Conservation of Electricity is, as far as analytical applications are concerned, the exact analogue to Carnot's Principle for Heat.

Paris, Faculté des Sciences, June 5

G. LIPPMANN

Apparent Decomposition of Sunlight by Intermittent Reflecting Surfaces

IT occurred to me that light might be decomposed by interrupting, with a reflecting surface, a ray of light in such a manner that the interruptions may be proportional to the wave-length period of any particular ray forming a part of a composite ray. The experiment is effected in the following way:—

A wheel having bright spokes (the large wheel of a bicycle answers well) is caused to revolve between an observer and the sun, so that a ray of light is reflected to the observer by a bright spoke; then, when 120 spokes pass before the observer per second, violet light shines out vividly; when 65 pass, red appears, and different rates of revolution give different colours. There seems to be a marked relationship existing between the number of spokes which pass by and the wave-length of the two colours mentioned, that of the violet being $\frac{1}{80000}$ inch, and that of the red $\frac{1}{40000}$ inch.

I am now investigating this apparent relationship between spoke-interruption and wave-length for the other colours of the spectrum of white light, and I hope to be able to make known the results shortly.

FREDERICK J. SMITH

Taunton, June 4

Symbolical Logic

I AM sorry that Mr. MacColl should have thought that there was any intention on my part to suggest a doubt as to his having

written his papers without having read Boole's "Laws of Thought." I knew that he was very anxious that the fact should be known, and I called attention to it. I could not state it as a fact known to me. His own assurance was the only ground I had, or could have, to go upon, and in assigning this it never occurred to me to doubt his statement, or to think that I was suggesting doubts to others.

As regards my half humorous suggestion that an attitude of slight social repression was desirable towards novelties of mere notation—not towards new conceptions or methods—I feel sure that almost every one who has not a private scheme of his own to protect will agree with me. Few things can be more perplexing to students of any subject than to find one author after another making use of a new notation to express old results (I mean no special reference to Mr. MacColl here, who does not seem to me one of the worst offenders in this way). At the time of writing my "Symbolic Logic" I had between twenty and thirty such schemes before me. Some of these, of course, express really distinct conceptions, or effect improvements in procedure, but most of them do not; we find half-a-dozen different signs standing for the same meaning, and half-a-dozen different meanings assigned to the same sign. I cannot but think that much of this confusion would be avoided if the various authors would take the trouble to inquire what had been already written upon their subject. The only "repression" I should like to see introduced consists in the remonstrances of reviewers and students generally against the mere substitution of a new symbol for one which was already in use for expressing precisely the same process or conception. So far from wishing to discourage any attempts to improve on the results of Boole and others, I rejoice to see them, and think that Mr. MacColl himself has done some good work in this way. It would have been better still if he had not disfigured it by a notation which I think makes him regard his results as more original than they really are.

I need not seriously discuss those parts of Mr. MacColl's letter which give his opinion as to the impression which will be produced in other persons by a perusal of my book, and his "impression" that he has "somewhere seen Mr. Venn quoted as holding an opinion very much at variance with" a statement which he misquotes.¹ (By the way, I heartily agree with his "protest against that spirit of criticism which would offer two or three chipped bricks as a fair specimen of a house," &c., and think the chipping of the bricks a happy turn.) The rest of his letter contains criticisms upon my conclusions on a variety of rather intricate speculative questions. Having stated my own views as fully and accurately as I conveniently could only a few weeks ago, in a systematic work, I really must decline to be drawn into repeating them again, in a condensed form, in the columns of a scientific journal, even if the editor would consent to accept them.

J. VENN

Cambridge, June 12

Telephones in New Zealand, &c.

OBSERVING your paragraph on this subject in NATURE, vol. xiv. p. 88, it occurs to me that the following may be of interest:—When in Wellington and Dunedin, N.Z., at the end of December last, my opinion was asked by the Government Telegraphic officials there upon a pair of ordinary "Edison-Bell Telephones" (not Edison *bell-telephones*, as they are too frequently called) which they had just received from the United States for purposes of experiment. A careful trial under various conditions showed me that they were very good average instruments of ordinary delicacy, such as I had seen hundreds of previously in England and the States.

With these instruments, however, Dr. Lemon, the Superintendent of the Postal and Telegraph Service, was able to converse clearly between Wellington and Napier, over an ordinary land line 232 miles in length, while battery currents were passing over the wires on the same posts.

In New Zealand, Telegraphic communication is, and Telephonic communication will be, entirely in the hands of the Government. In Melbourne the telephone-exchange is worked by a private company, but the erection and maintenance of wires is carried out by the Victorian Government at the annual rate of 5*l.* per sub-

¹ What I spoke of was "those problems in Probability which Boole justly regarded as the crowning triumph of his system." What Mr. MacColl puts between inverted commas is that Boole "justly regarded his problems in Probability as the crowning triumph of his system," and challenges me to say whether or not I agree with Boole's solution of a certain well-known example. This considerably distorts the meaning of what I said.

scriber. In Sydney, I regret to say, nothing was being done in this matter. In Honolulu I found (last January) telephonic communication all over the town, but no telegraphs at all. The King of the Sandwich Islands however, Alii Kalakaua, who is shortly expected in England, told me that he greatly needed submarine cables between the various islands. On my return to England I had the pleasure of sending to Sydney materials for a private telephonic line on sugar plantations in the Fiji Islands, and my friend Mr. Frederick Cobb, manager of the Falkland Islands Company, tells me that the line he took out there at my suggestion is a great success.

At Wellington, where the central N.Z. telegraph office is, I was very much struck by the extreme ease with which duplex circuits were worked. Dr. Lemon informed me that it was scarcely necessary to alter the resistances once a week. He showed me a simple little carbon rheostat of his own invention which appeared to answer admirably; it consisted essentially of two pieces of carbon, the closeness of whose contact was regulated by a screw.

On my way home I paid a hurried visit to the central office of the Western Union Telegraph Company in New York (just at the critical time of the absorption by it of the other two companies and the consequent creation of a monopoly), and was greatly surprised to see the extent to which the 16,000 cells in the battery-room were being replaced by Siemens's dynamo-machines. I was told that one of them would "drive" about fifty wires, and was shown a number of plaster-of-paris cylinders, about five inches long and one inch diameter, which were put into circuit to diminish, when necessary, the intensity of the current. It may be remembered that as a rule American lines are less perfectly insulated than ours, and hence require stronger currents.

WM. LANT CARPENTER

6, York Buildings, Weymouth, June 1

Implements at Acton

MR. PERCEVAL's letter in NATURE, vol. xxiv. p. 101, is an interesting one, but the occurrence of Neolithic implements at and near Acton has been known (if not published) for many years past. In the Pitt-Rivers' collection may be seen Neolithic scrapers and flakes from the Acton district. I have found Neolithic stones in the neighbourhood of Acton and Willesden for many years past; and only a few weeks ago I picked up a beautiful and perfect knife of black flint made from a large flake, five and a half inches long, and one and three quarter inches wide, in the field on the east of Acton Station of the North London Railway. Many of the Neolithic flints from this position are white. A considerable number of Neolithic implements and flakes have at different times been dredged up from the Thames to the West of London, and some of these have been quite recently exhibited. I do not attach importance to the quartzite pebble, as pebbles of quartzite are extremely common in the glacial deposits at the North of London, and very common in the gravels of the Thames and its northern tributaries. They also occur *in situ* at the north of Willesden.

Will Mr. Perceval kindly furnish the heights at the Hammersmith position, and say whether he is positive that the gravel he has in view was dug on the spot, and whether the implements occur there (as his letter implies) in "remarkable abundance"? I have repeatedly examined the low gravels about Hammersmith, Fulham, and Chelsea, but with no result. For more than three years I have never missed an opportunity of looking over the low gravels belonging to these places, together with the positions at West Brompton and Kensington, where thousands of tons of gravel have been excavated. My result has been one dubious flake, probably washed down from one of the higher terraces. I however have heard of two Palæolithic implements having been found—one at Kensington and the other at West Brompton—but whether from the local gravel or not I am uncertain.

I by no means wish to imply that because I have been unable to find implements in the lower gravels therefore some one else may not have found them. Some one may have been always before me and picked them up, or I may have constantly looked over unproductive patches.

The places mentioned by Mr. Perceval are, it must be remembered, frequently ballasted with gravel brought from a distance by the Thames, by the Grand Junction Canal, and by the Great Western Railway. I know of at least five different localities whence the Acton and Hammersmith gravel is brought, one